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EIGHTY SIXTH SESSION.

Monday, 23d November 1868.

DAVID MILNE-HOME, Esq., Vice-President, in the Chair.

The following Council were elected :—

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HIS GRACE THE DUKE OF ARGYLL.

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Dr WILLIAM ROBERTSON.

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VOL. VI.

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Monday, 7th December 1868.

Professor Christison, the President, delivered the following
Opening Address:—

GENTLEMEN,—It is now nearly five-and-forty years since I first opened my lips in this Society, a venturesome young man, undertaking to instruct both practical men and men of science, as well as the public at large, all at the time keenly interested in the inquiry,—What were the principles for properly constructing burners for combustion of the light-giving gases? What the relative values of oil-gas and coal-gas for giving out light, and which of the two should thenceforth be used for illuminating the world? At that time it was assuredly the farthest thing of all from my dreams that a period might come round when, by the voices of my Fellow-Members, I should be promoted to an office, the highest in learning or science, as the case may be, which Scotland can offer to their votaries,—held last by Sir David Brewster, and previously filled by the Duke of Argyll, Sir Thomas Brisbane, Sir Walter Scott, and Sir James Hall.

All these eminent men have been Presidents of the Royal Society of Edinburgh during the period of my Fellowship, except the last: whom, however, I often met in the literary and scientific circles of which my father was a member. When I recall the remarkable services of these five men to learning and science; their contributions to the meetings and Transactions of this Society; their bearing among its Fellows, and the universal homage paid to them as our official heads,—I am apt to ask myself why it is that I am now in the place previously adorned by such men as these? To this question I have but one answer, and it may be an inadequate one. The office was not of my seeking. It was not within the range of my hopes, or even of my thoughts. But when my appointment to the Presidentship of the Society was spontaneously recommended to the Society by the Council, I felt that I could not but bow to their decision, as being the persons best qualified to know what is most for the advantage of the Society, and having undoubtedly reasons, sufficient to their own minds, for placing in me, for the due discharge of the duties of your President, a confidence which

I can truthfully say I do not myself entertain. And nevertheless, having undertaken these duties, and possessing some knowledge of what they are, I must hope to discharge them to your satisfaction, with the help of a willing mind, a grateful heart, and your indulgence.

While pondering over these reflections, it occurred to me to inquire whether any mark had been left on the work of this Society by my several predecessors in office. It was, I confess, some comfort to me to find that at all periods the work done in the Society depended more on the Vice-Presidents, the Secretaries, and the other members of Council, as well as the Fellows at large, than upon your President for the time being; who may have felt, perhaps, that, having generally done good work previously in an inferior capacity, he might be allowed to repose a little on his dignity, and transfer much of the arduous labour of literary and scientific research to those enjoying the inestimable advantages of greater vigour of life, and a younger enthusiasm.

In the course of my searches into this matter, I had to go farther back than my personal recollections, even to the Presidency of Henry, Duke of Buccleuch, and thus was imperceptibly led into the midst of the earliest proceedings of the Society, when it first started into existence in 1783 as the Royal Society of Edinburgh, under the leadership of that distinguished nobleman. The study of the Proceedings and Transactions of the Society during his Presidentship, which lasted for the long term of twenty-eight years till his death in 1811, seized upon me with a fascination which no one can thoroughly understand without following my example. But it has seemed to me that I might convey some portion of the pleasure of that retrospect were I to summarise and cull the interesting and very diverse materials thus presented to view, and offer you, as my Address on the present occasion, a history of some portion at least of the Life of the Royal Society of Edinburgh.

The History of our Society has never yet been written. But it is a duty owing to our predecessors, who earned for it a great reputation, that some time soon such a history should be written. The task, however, is one which will need the labour of several hands—the materials being, as already said, so very multifarious.

All I can pretend to do, with the small ability and little leisure of which I can avail myself, is to attempt, in a desultory way, as much at present as my time admits of my overtaking, and yours of your listening to. But I have some hope, nevertheless, that so much may be said as to induce my able coadjutors, who annually in succession address you from the Chair, to look also a little into the materials I have been examining, and to show, by each taking up that branch of our history which is most allied to his pursuits and congenial to his inclinations, the various branches of learning and science on which our Society has more or less shed its rays during the last eighty-five years,—the most eventful era, probably, that has occurred in the literary and scientific history of this nation.

It is scarcely necessary to remind you that the Royal Society of Edinburgh was created mainly through the influence of Principal Robertson, from a pre-existing association of the most illustrious Scotsmen of the time, called the Philosophical Society; which again had arisen, through the exertions of Colin Maclaurin, from a combination of learned and scientific men who published between 1731 and 1739 the “Medical Essays and Observations.”

The Royal Society, in its early years, was chiefly composed of the following remarkable galaxy of genius and talent:—Between 1783 and 1805 we find, in the branch of *Literature*, and most of them taking their share in the business of the Society, the names of Principal Robertson, the Rev. Drs Hugh Blair, Carlyle, and Henry; Professors Hill, Dalzel, Ferguson and Fraser-Tytler of Edinburgh, Beatty of Aberdeen, Hunter of St Andrews, and Young of Glasgow; Dr Doig of Stirling, Henry Mackenzie, and William Smellie. In *Philosophy*—Adam Smith, Thomas Reid, Dugald Stewart, and John Bruce. In *Mathematics* and *Physics*—Professors Matthew Stewart, John Robison, Robert Blair, John Playfair, John Leslie, and William Wallace, of Edinburgh; John Anderson of Glasgow, and James Ivory. In *Chemistry*—Black, Rutherford, Hope, Roebuck, and Lord Dundonald. In the *Natural Sciences*—Professors Walker and John Hope, Hutton, Playfair, and Colonel Imray. In *Medicine*—Cullen, the second Monro, James Gregory, Francis Home, Andrew Duncan, Sir Gilbert Blane, and Benjamin Bell; and to these must be added a host of conspicuous lawyers

and country gentlemen of the time, all more or less conversant with literary or scientific pursuits, among whom the most conspicuous were Lord-President Dundas, Lord Justice-Clerk Miller, Lords Meadowbank and Glenlee, Barons Montgomery, Norton, and Hume, Sir Henry Campbell, Sir James Hall of Dunglass, Sir George Clerk Maxwell of Penicuik, Sir Alexander Dick of Prestonfield, Sir George Mackenzie of Coull, Sir James Hunter Blair of Dunskey, Sir William Forbes of Pitsligo, and John Clerk of Eldin.

It will be acknowledged to be difficult to discover in any country, and at any period, united in a single association, sixty names more remarkable than these for the impression they have made on the peaceful part of the history of a nation.

The medical element of the Society was in those days peculiarly rich in great names. This was the element, too, from which the Society originally sprung. We should therefore naturally expect medicine to flourish in a Society descended from the medical essayists of the middle of last century. But medicine makes only a rare, and for the most part insignificant appearance in the business of the Royal Society. It is a matter of curiosity, however, to see how, even so far down as 1792, when very much smothered by literature and pure science, medicine still sometimes cropped out in naked simplicity. Among the early papers read in the Society, Dr Hope describes a disagreeable case of death from impaction of a gall-stone in the bile-ducts; Dr Butter of London proclaims hemlock to be a sovereign cure for St Vitus's dance; Dr Duncan intimates that he had cured an inveterate hiccup with a single dose of diluted sulphuric acid; at a later period, Mr James Russell, afterwards Professor Russell, a well-known colleague, communicates a singular case of hernia, and Dr Francis Home treats of the disease Amaurosis. If this be all which medicine could do in its most palmy days in Edinburgh to hold up its head in the Royal Society, I confess it is not a subject of regret that, by gradual and tacit consent, papers on pure medical practice have been allowed to drop from our Proceedings. For assuredly there is nothing at all so remarkable or peculiarly instructive in death from an impacted gall-stone, or from any form of hernia, as to deserve being re-

corded in the Proceedings of a Royal Society; nor would I advise patient or physician to trust much either to Dr Butter's cure for St Vitus's dance, or to the remedy which seemed to Dr Duncan to put an end to inveterate hiccup. The first volume of our Transactions, however, contains one able paper on a subject of pure practical medicine—a treatise by an eminent English physician of last century, Dr Hamilton of Lynn-Regis, upon the Mumps, or Cynanche parotidæa, a disease which had previously been little studied in England. But this treatise was originally produced to the Philosophical Society in 1773, and the Royal Society, deeming it worthy of publication, printed it in the Transactions.

In the more congenial fundamental sciences of medicine, anatomy and physiology, the work done in the Society in its early years presents features of greater interest. Mr Blizard of London gives an account of an extra-uterine foetus; Dr Monro describes a remarkable male monster; investigates the anatomy of hydrocephalus; inquires into the communications existing between the ventricles of the brain—a field in which he had long before been a famous discoverer; explains the action of those muscles which consist of oblique fibres; and narrates the results of experiments, among the earliest made in this country, in 1792, confirmatory of the immortal discovery by Galvani of galvanic, or, as it was then called, animal electricity. The investigations of Dr Francis Home on the relative strength of tonics, and his medical experiments on foxglove, read in the Society, but published in a separate treatise, show that there is another fundamental branch of medicine proper—therapeutics, which may be cultivated, as a science, by physiological experiment, as well as by mere observation of empirical facts; and until this view be taken generally by physicians of the true method of studying the action of remedies, medicine will make but little progress in this the most important of all its departments.

There are other medical inquiries of interest to be found in the Society's early Proceedings and Transactions, such as Dr Alexander Wilson's paper on the action of opium on animals, and that of Dr James Johnston on the functions and diseases of the lymphatic glands. But I hasten from these and other inquiries, which might be usefully put in detail before a medical audience, to other topics of more general interest.

Before doing so, I will venture to touch for one moment on a rather delicate topic—the connection of the Society in its early days with theological subjects of discussion—because I find at that time an incident which might have helped the Council and the Society when a question was lately raised as to this matter. In 1791 and 1792 the Rev. Dr Ogilvy of Midmar read no less than three successive papers on the Theology of Plato, whereupon there occurs the following minute:—"The Society observed with regret that the discussion of a religious nature, contained in this learned communication, rendered an admission of it among their papers inconsistent with their plan; and therefore it was not put into the hands of the Committee for publication." If it was found necessary to discourage such discussions in 1790, it is not less so in these days, when religious differences are not less general, diverse, and keen. Appealed to on a recent occasion to state the law or usage of the Society on this subject, the Council had only one difficulty; which was to define what amounted to the nature of a theological communication. It would be clearly absurd, for example, to say that when a scientific observation or inquiry threw light upon a scripture fact, or some incident in scripture history, the author should be debarred from calling attention, in passing, to such a practical application of his inquiries. On the other hand, no Fellow of the Society would probably ever dream of introducing into it an express dissertation on a pure theological theme. Between these extremes there must be a limit which ought not to be crossed, and which it must be left in the first place to every man's discretion to observe; but if it be passed, I apprehend that it is still the duty of the Society, or its Council acting by its authority, to see that the offending communication, as of old, "be not put into the hands of the Committee for publication."

The first twenty years of the life of the Society is rich in literary inquiries. I venture to hope that some time soon, a literary Vice-President will undertake to tell us something of these communications, both on account of the information they contain, and for the sake of the light they throw upon the character, in point of learning, of their authors, who were among the most learned Scotsmen of the period. It appears that no fewer than thirty-eight papers on

literary subjects were read at the Society's meetings, and are in general either summarised or published at length in the first six volumes of the Transactions. The most material are ten Grammatical papers by Dalzel, Hill, Hunter, Young, and Gregory, chiefly; one Etymological by Dr Jamieson; three Critical, on the *Æneid*, by Beatty, the character of Hamlet by Dr T. Robertson, and the German theatre, by Henry Mackenzie; three on Historical Composition by Hill and Fraser Tytler; three General Dissertations connected with the subject of poetry, by Dalzel and Hill; on the Standard of Taste, by Dalzel; on the Principles of Translation, by Fraser-Tytler; on the Argonautic Expedition, by the Rev. Mr Marshall of Cockpen; on the Origin of the Hellenes and of their name, by Dr Doig, of Stirling, who traces the Greeks to Chaldea; on Written Language as a Sign of Speech, by Dr Hutton; four Ethnological papers,—1. By Dr Gregory, on a Lacustrine Fort in Loch Urr, Kirkcudbrightshire; 2. By Colonel Montgomery, on an Ancient Sculptured Stone in Coilsfield; 3. By Fraser-Tytler, on the Vitrified Forts in the Highlands; 4. By Dr Anderson, on the Ancient Circular Buildings, or Houses of Scotland; and last, and one of the chief of all, the elaborate researches of Chevalier into the Plain of Troy, read by the author himself to the Society in 1791, and published *in extenso* in the French language, in the third volume of the Society's Transactions. I must leave to the literary members of your number to say, whether there is not, in these diversified papers, rare materials for such a historical summary of the work done at our meetings of old as I have indicated. I myself can only aim for the present at a brief allusion to the remarkable investigations of Chevalier.

Doubts have often been raised, and keen controversy has arisen among the learned, as to the reality of the existence of a Homer in poetry, an ancient Troy in geography, and a Trojan war in history. Chevalier, by his careful personal survey of the plain of Troy and its neighbourhood, contributed much to dispel these doubts, in so far as he ascertained that the leading incidents described in the *Iliad* fall in exactly with the remarkable structure of the Plain and surrounding country; thus furnishing internal evidence that the composer of the *Iliad* must have been intimately acquainted with the district where the Trojan war was carried on. It is only in the

most recent times that writers of romance have thought it worth their while to know and portray the natural features of the places where the incidents they invent are represented to have occurred. It is a matter of doubt, whether even the most recent and scrupulous works of the kind could stand successfully so minute a topographical scrutiny as that to which the *Iliad* has been subjected by Chevalier and others since his time. But it is scarcely to be supposed that a composer of mere fiction, in so remote a period of antiquity and barbarism, and when intercommunication between countries was rare and difficult, would, as must be held by the advocates of the *Iliad* being a romance, undertake so troublesome and unnecessary a condition in those days, for a work of the imagination, as a minute personal study of a wide and complex field of strife, which he chose for the theatre of a romance. It is impossible, therefore, to deny that the researches of Chevalier added greatly to the pre-existing probability that Homer was an entity, and the siege of Troy an event, in real history.

The main features of Chevalier's *Tableau de la Plaine de Troye*, remain undisputed to the present day, save one, and rather an important one, the actual site of ancient Troy itself. Our late much-esteemed Fellow, Mr Charles Maclaren, without having visited the plain (though he did so subsequently), called in question the conclusion at which Chevalier arrived as to the position of the town for the possession of which Homer's heroes fought. The district may be briefly described as a plain about eight miles long and five miles in breadth, stretching from the westerly spurs of mount Ida to the Ægean Sea, bounded on the north and south by ridges of moderate elevation, and separated from the Ægean by a line of low heights, except at the mouth of the river Mindere or Simöis. Chevalier placed the site of ancient Troy near the south-east angle of the plain, in the lower region of the bounding ridge on the south, and describes traces of ancient walls in this locality, close to a modern Turkish village, Bounarbashi. But Maclaren points out that this position,—nine miles from the coast, where the Greeks were encamped, with their vessels drawn up on shore,—is much too great for the time allowed by Homer for certain incidents of the war which took place between the two limits, the town and the ships; and bringing to his aid the knowledge of a skilful geolo-

gist to explain changes, evident to such an eye, which in the course of ages may have taken place in the channels of the rivers and the structure of their banks since the time of the siege, he arrives at the result, that the site of the ancient town was the same with that of Strabo's *Novum Ilium*. There is a long tongue of wide, elevated land, steep on the sides, but not precipitous, which stretches from the lower heights of mount Ida for more than two miles westward into the very heart of the plain of Troy. The western terminal portion of this ridge was the site of *Novum Ilium*; the ground-plan of which is still indicated by remains of walls, and by such an accumulation of broken bricks and shattered porcelain all over the surface, that when friends of mine visited the place, while in the army medical service at Renkioi Hospital, during the late Turkish war, they found it a troublesome matter to measure the ground, by pacing through the covering of loose fragments. Now, it is scarcely to be conceived that, when the people of the country, represented by Homer to be well acquainted with the art of defensive fortification, determined to erect a fortified town for the protection of their territory, they would overlook this conspicuous spot, eminently suitable for all the conditions of refuge and defence in these early times, and would choose rather a remote corner of their land, leaving the whole cultivated plain between it and the sea open to sudden piratical incursions. But on other grounds than this Mr Maclaren has proved, to the satisfaction of all scholars who have studied the subject since, that Troy and *Novum Ilium* must have stood on nearly the same site. I may mention, as a new argument in support of that view, that Dr Kirk, one of the visitors referred to, found Chevalier's supposed foundations of ancient walls at Bounarbashi to be really the remains of trap-dykes.

So far Chevalier has been corrected. But the criticisms of Maclaren do not take away from the light which Chevalier was the first to throw over the most interesting of all events described as occurring in remote secular history.

Among the literary labours of the Society must be arranged a crowd of authentic biographies of the most eminent Scotsmen of the time, Fellows of the Society, written by men generally not less distinguished than those whom they have commemorated. This is a branch of the Proceedings of the Society which it is im-

possible to summarise. But I have alluded to it on account of a very surprising statement in the biography of Sir George Clerk Maxwell of Penicuik, father of our lately deceased Fellow, Sir George Clerk, which was read in 1784 by John Clerk, junior, of Eldin, afterwards Lord Eldin of the Scottish bench. The author says, Sir George "had an excellent taste for the fine arts, and was solicitous to encourage them. As one instance of this, he had the principal concern in establishing and procuring an endowment for the drawing-school in the University of Edinburgh, where twenty pupils are instructed *gratis* in the art of designing. These are selected from among such young people of either sex as give signs of genius, who are destined to apply to those professions in which a skill in that art is requisite. This institution has contributed more than any other circumstance to the great improvement of ornamental manufactory which this country has made during the last twenty years. And who ever recollects the old patterns of carpet, damask, gauze, and other manufactures of that sort, and compares them with those of the present day, must allow the superior elegance of design now exhibited in these productions, and which may be reasonably ascribed in a great measure to the happy effects produced by the institution we have mentioned."—*Trans.* i. 51.

It is impossible that Mr Clerk, himself an ardent admirer of art, and addressing a Society composed largely of Professors of the University, could be mistaken in making this precise statement. Nevertheless, I never heard of such a school in the University. There was most assuredly no such school in existence when I first joined it as a student in 1811. It cannot have merged in the present excellent School of Design, because that school, as now constituted, was founded only in recent times; and to several of its governors this passage from Sir George Clerk Maxwell's biography has seemed quite as novel and surprising as to myself. What, then, has become of the University School? When did it expire? How did it vanish? Above all, what has become of the endowment? All I can say upon the last head is that positively it has not been swallowed up by the Senatus Academicus since I became a member of that body in 1822.*

* The statement of Mr Clerk seems to be explained by an observation, to

Philosophy seldom ventured to appear before the Royal Society. In 1784 Dugald Stewart read an essay on Cause and Effect, and in the same session Dr Gregory read another on Cause and Effect in Physics. But of neither is there even an abstract in the Society's Proceedings.

The Chemical papers produced before the Society during the first twenty years of its existence—sixteen in number—are not so numerous as we might expect, when it is considered that the great discovery by Black of the composition of the carbonates, and the nature of carbonic acid, was still of rather recent date. But some of these papers are of interest, and retain their value in the present day. The most important of them are as follows:—

That remarkable genius, Lord Dundonald, father of the lately deceased naval hero, read in 1784 a characteristic inquiry into the purification of sea-salt. Having found that a concentrated solution of common sea-salt possesses the property of dissolving a large quantity of the magnesian salts, which are always to be found in it in small proportion,—rendering it, however, attractive of moisture in damp air, and less fit for the curing of meat,—he placed the salt to be purified in a conical filter, and passed through it two or three successive portions of a concentrated hot solution of the same impure salt, with the effect of removing almost entirely the salts of magnesia. This method was plainly applicable to the large manufacturing scale; and the inventor had only, as in the purification of loaf-sugar by pure syrup, to displace the last saline liquor remaining in the interstices of the salt, by a solution of pure chloride of sodium, when the whole of his product would have been quite pure.

Drs Black and Hutton being appointed with Mr Russell a committee to examine and report on a process proposed for manufacturing spirit from carrots by two enterprising experimentalists, Hunter and Thornby, they find that twenty tons of this root yield

which my attention has been turned in Hugo Arnot's "*History of Edinburgh*" (1789, p. 323), that the Board of Trustees for Encouragement of Manufactures appointed an artist in 1772 to teach twenty boys or girls drawing, and obtained for the purpose, from the Town Council, the use of two rooms in the University. (Dec. 14.)

two hundred gallons of proof-spirit, equal in quality and cheapness to the best sort of corn-spirit. It is easy, through action of rectified spirit on extract of carrot-juice, to crystallise from it fine cane-sugar, which will account for this result. But the Society's records do not state anywhere what success the inventors attained with their new manufacture.

Dr Kennedy, well known and esteemed at the period as a chemical analyst, contributes a careful analysis of a new Zeolite from the greenstone of Salisbury Crags, showing that it consists almost entirely of silica, lime, and $8\frac{1}{2}$ per cent. of soda; and in 1798 he communicates a much more interesting account of the composition of basalt, greenstone, and lava. In the latter paper he comes to the conclusion, that the basalts and greenstones around Edinburgh, and the lavas around Etna, all agree both in the nature and the proportion of their main constituents, which are silica, alumina, and oxide of iron, with, in all, about 4 per cent. of soda. The interest of these facts, in their application to the prevailing geological doctrines of that period, as well as of the present time, will be seen presently, and, indeed, is obvious on the bare mention of them.

In 1791 Dr Black presents his last communication to the Royal Society, his famous analysis of the spouting hot springs of Geyser and Rykum in Iceland. In the Transactions this important paper is accompanied by another from Mr Stanley of Alderley, M.P., from whom Black obtained the waters for examination, and who himself collected them during a visit to Iceland in 1789. There has appeared since that time no better account of these extraordinary springs than that given by Mr Stanley, and in some respects it surpasses more recent narratives in complete and graphic description. I know not, indeed, that any subsequent visitor has added much to our knowledge until our deceased Fellow, Mr Alexander Bryson, communicated to this Society his excellent thermometric observations on the very high temperature attained by the waters at great depths in the funnel whence they are projected. In Black's time geologists and chemists were puzzled to account for the solution in the water of the great amount of silica necessary to form the extensive deposits of hard siliceous sinter around these fountains. Black's analysis gave the explanation. Of the

eight grains in the Rykum water, and ten grains in the Geyser water, of solids in 10,000, Black showed that about one-half consisted of silica dissolved by soda; that the soda amounted to half a grain in the former, and a whole grain in the latter; that it was the means of dissolving more than six times its own weight of silica; and he conjectured that this great dissolving power was partly communicated by very high heat, existing where the process of solution was constantly going on.

In 1793, in a postscript to the Proceedings of the Society for that year, Dr Hope produces his well-known discovery of the mineral strontianite, his analysis of it, his discovery of the new earth, strontia, and his investigation of the properties of the earth itself and its compounds. This paper, which at once established for him a great name among chemists, and was given to the world when he was quite a young man, supplies internal evidence that Dr Hope possessed in an eminent degree all the qualifications for a profound analyst and discoverer—patience, inventiveness, accuracy, and acute discrimination. The wonder is,—which all his scientific friends have felt, but no one has thoroughly explained to his own satisfaction,—that with this first-rate investigation, and at the mere outset of scientific life, Dr Hope's career as an analyst both began and ended. New earths, new alkalies, new acids, new metals, were constantly announced from all sides around by those engaged on the same field which he had shown he could fruitfully cultivate. But Dr Hope never undertook another chemical analysis.

In 1788 Sir James Hall read an exposition of Lavoisier's new Theory of Chemistry, being probably the first account given in Scotland of that philosopher's great discoveries. Soon afterwards Hutton read a reply to Lavoisier and Sir James Hall, in an essay on Phlogiston. Both papers are lost to the world; for, at the time when they were read, authors did not desire to print, and the Society did not publish, either in its Proceedings or its Transactions, scientific papers of a merely critical nature. We can easily understand, however, how stoutly Hutton would at that period stand up for the existence of phlogiston; which, nevertheless, was doomed to die a sudden death at the hands of the French executioner.

In 1796 Dr Hutton, in a paper entitled "A New Phenomenon

in the Sulphurating of Metals," proves that when sulphur and iron are heated together, and combine to form the sulphide of iron, the process is one not of inflammation, as might appear on a hasty survey of the phenomenon, but of incandescence, which lasts only during the short time combination goes on, and without any of the essentials of combustion. He annexes some inferences in respect to the connection between heat and light, which I may have to revert to under a future head, as in some measure anticipating a great modern doctrine. The experiment, a striking one, has been rendered long familiar in consequence of Dr Hope, who assisted Hutton in this inquiry, having constantly made it the subject of brilliant demonstration in his lectures, as many of us must well remember.

In 1800 Sir George Mackenzie presented to the Society a very conclusive inquiry as to the combustion of the diamond. It had been burnt, and its nature deduced from the product, in various complex ways, by several eminent chemical philosophers. But Sir George had the merit of showing how it might be consumed in a simple muffle with unaided heat at 15° of Wedgewood's pyrometer,—that steel might be made by heating iron in contact with its dust,—and that, by duly heating a mixture of pulverised iron with a fourth of its weight of diamond dust, a fused mass may be obtained which is quite undistinguishable from cast-iron.

I conclude these chemical notices with a second original investigation of great excellence and interest by Dr Hope. It has been long well known that, as water cools down from a mean temperature, it contracts, and consequently the cooled particles sink until the thermometer indicates about $39^{\circ}5$ Fahr.; that on farther cooling, however, this general law which regulates the cooling of liquids is upset; that the water then actually expands, and consequently the cooled particles now rise; and that this expansion continues, till at 32° a much greater expansion suddenly takes place, when the water is converted into ice. The contrary phenomena, of course, occur as water heats from 32° . It contracts till the temperature rises to $39^{\circ}5$, and after that expands according to the general law. But the fact of the density of water thus deviating from the general law which governs the influence of heat on liquids, had been often called in question by high

authorities in chemical physics; and among those who acknowledged the reality of the deviation there was irreconcilable disagreement as to the temperature at which in the descending scale the deviation begins. Hope in 1804 settled both points by a set of admirably contrived experiments, in which he took the temperature in various parts of a long column of water,—at the bottom, at the middle, and near the surface,—cold or heat being applied variably at these several points to produce the necessary intestine movements among the particles of the water. The whole paper on this subject is an admirable example of experimental reasoning, which definitively settled, as we now know it, a great fact in nature's laws of much practical value in the economy of this earth, relative to the freezing and thawing of water, and the influence of these changes in its condition on the air, the land, and animal life. This was the second of Hope's original researches, and the last; and again our astonishment is raised that it should have been the last.

In the highest walks of Mathematics and Physics, the Proceedings of this Society have always abounded with important investigations. No one can doubt that such must have been the case when Robison, Playfair, Leslie, Ivory, and Wallace took a large share in the Society's business. But this is a branch of my subject to which I cannot myself do full justice; and time has not sufficed for me to complete my undertaking by asking aid from my well-qualified colleagues, who, I am sure, would have cheerfully granted me their assistance. The papers are chiefly researches in geometry and algebra of the most abstruse kind. But there are two of a different stamp—on the confines between chemistry and physics—which may be here shortly noticed, as they are the work of one whose labours will call presently for a larger share of our attention.

In 1794 Dr Hutton communicates a dissertation "On the Philosophy of Light, Heat, and Fire." In this inquiry he reasons against the existence of radiant heat, apart from light, which had been announced recently before as an important discovery by Saussure and Pictet. Hutton maintained that the real radiation is of "invisible light"—light so faint as not to be cognisable by

our senses. In the course of his argument he advances the proposition that light is the "immediate cause of burning," and that the light which appears in combustion is "the extrication of phlogiston, fixed light, or a certain modification of the solar substance which had existed in the inflammable bodies, chemically united with their elements." Finally, he winds up with the doctrine that "light, heat, phlogiston, and electricity, are so many different modifications of the solar substance." Converting these "substances" into qualities of matter, and striking out the lingering remnant of phlogiston, we have here the modern doctrine that heat, light, and electricity are mere varieties of the same quality of matter, and that the Sun is the primary source of them all.

Theother chemico-physical paper is "on the Force Exerted by Water in Freezing," deduced from some experiments made in Canada in 1786 by Major Williams of the Royal Artillery. He exposed to intense frost a thirteen-inch shell filled with water, and plugged by an iron bolt. The metal of the shell was about an inch and a-half in thickness round the fuse-hole, and two inches and a quarter opposite the hole; and the plug, which weighed two pounds and a half, was driven into the hole with great force. At the moment of freezing the plug was driven out with such violence as to be carried 325, 387, and even 415 feet; and at the same moment a column of ice was thrust out of the fuse-hole of the length of four, six, and even eight inches and a half. If the plug, however, was secured by means of springs, like spiking nails, the shell was split, and the ice was thrust out in plates from the fissure.

From its first foundation the Royal Society of Edinburgh has been lavish in its contributions to the several departments of Natural Science. At least forty papers were read during the first twenty years, and many of them have been published in the Transactions, on the branches of Zoology, Botany, Topography, Meteorology, Mineralogy, and Geology.

In the branch of Zoology, however, there was extreme barrenness at that period in the Society. Mr Kerr, in 1790, notices an "Animal ignotum" in the University Museum; and this is all we learn of the animal. That astounding personage, the Great Sea Serpent, makes his appearance once on our boards, under the

patronage of Mr John R. L'Amy, Justice of Peace for Forfarshire, in the character of a Kraken, three miles long, as seen at the statutory distance of about one mile by a credible master-mariner and his mate, off the east coast of Scotland, on 5th August 1786. This is all that the Society contributes before 1803 to the science of Zoology.

Belonging to Botany there are only six papers, of which five possess interest. Dr James Anderson of Madras describes, in 1791, the *Oldenlandia umbellata*, from the root of which is obtained in India a valuable red dye-stuff. The Transactions for 1785-90 contain an excellent paper, with illustrative drawings, by Dr Wright, Physician-General of the Army in Jamaica, on the *Quassia simaruba* of that island. The root of that species, now the *Simaruba officinalis*, was strongly recommended by him as a remedy for chronic dysentery, and has still great credit with many in the treatment of that disease. Dr Wright was the first to identify and accurately describe the tree, and did so in this paper, which was read in 1778 in the Philosophical Society, but was first published in the second volume of the Royal Society Transactions.

In 1791, Mr John Lindsay, surgeon in Jamaica, communicates a paper connected with the subject of the last-mentioned inquiry, on the *Quassia polygama* of Jamaica, describing the plant as a magnificent tree 100 feet high and 10 feet in girth, and representing it to possess all the virtues of Quassia wood, the produce of the *Quassia amara* of Surinam. He adds, he is credibly informed that the former is sold in London for the other. This is a curious fact, as fixing the time when the true Quassia wood of the *Quassia amara* of existing botanists was displaced in the markets of Europe by the wood of Lindsay's tree, the modern *Picræna excelsa*, without either druggist or physician having noticed any difference in their virtues, or observed, until a few years ago, that the wood of a great forest tree had been substituted for that of a low bush about twelve feet in height. Different views may be taken of this apparent blindness of the medical profession. For my part, I recognise in the whole incident an interesting proof of the resemblance in action on the human body among different plants belonging to the same natural family. For the great tree of Jamaica, like the little bush of Surinam, is a powerful, simply-

bitter, tonic stomachic; and I believe the two may be used indifferently for all medicinal purposes. Mr Lindsay is not so fortunate in a notice he has given in the same paper of *Cinchona brachycarpa*, a new Jesuits'-bark tree, and cure for intermittent fever. This is now known to be no true cinchona at all, and to be immeasurably behind the true cinchonas as a febrifuge. The tree is now the *Exostemma brachycarpa*; and, like all other species of *Exostemma*, its bark is more emetic than anything else in point of action—a property which, it is fair to say, had been recognised in it by Mr Lindsay himself.

Dr Hutton described in 1778,* and endeavours to explain, a phenomenon of vegetation on Arthur's Seat, which still remains open to further inquiry. It is well known to those who frequent the upper regions of the hill, that on various parts of the slope towards the east are to be generally seen grey zig-zag stripes on the grass, very conspicuous if the general herbage be fresh, almost always tending downwards, from a foot to two feet in width, continuous in some places, but interrupted in others, and stretching for many yards, occasionally for more than a hundred yards. On examining these marks, the grass is found to be completely withered to the roots; the roots themselves are destroyed; and many years elapse before the vegetation is restored. They are most frequent and well-marked in the hollow to the south of the basaltic summit, descending to Dunsappie Loch, but rather to the right towards Duddingston, and also on the subsidiary broad eminence north-east from the summit. Dr Hutton tries various theories for explaining these "Fairy footpaths," but cannot satisfy himself with any of them. Among the rest, he rejects lightning; which, however, I suspect is the only agent which will account for them. Many years ago, when wandering over the upper part of the hill at midsummer, I remarked that these marks were unusually scanty and imperfect. A thunder storm brewing in the south-west compelled me to effect a hasty retreat; and was followed by a very severe storm which passed over the hill, the city, and all the surrounding country. A few days afterwards I found the east slopes of the hill presented many extensive, recent-looking marks of the nature now described.

* To the Philosophical Society. Published in Roy. Soc. Trans. vol. ii.

The remaining Botanical inquiry I have to notice is one well known to naturalists, and of more general interest than the preceding. Great doubts had been long entertained among good authorities about the nature of the motion of the sap in trees. Dr Walker, Professor of Natural History in the University of Edinburgh, undertook a series of well-devised and precise experiments to determine how the sap moves in trees in the spring; and having with philosophical caution repeated them in several successive years, with the same results, he communicated the whole inquiry to the Royal Society in 1783 and 1785. This inquiry is still held to be authoritative proof, that the movement of the sap in trees, on the arrival of genial weather in the spring, is not a movement of circulation, by ascent and descent, as many had before contended, but invariably a simple movement of ascent; that the sap ascends neither in the pith, as some had maintained, nor in the bark, as insisted on by others, but in the wood, and between the wood and bark; that the date of its commencement, and its rate, both depend on the earliness and geniality of the warm season; that the ascent varies in rate, from six to nine inches daily, according to the prevailing temperature of the air; and that those buds always open first into leaves which the sap first reaches, so that its arrival is the essential cause of their growth. The author, however, points out that his experiments fix only the nature of the motion of the sap in the spring, when the tree has no leaves, and takes no account of what the movement may be when the leaves are developed. He is inclined, indeed, to presume that it may then be different; and accordingly ulterior inquiries have shown that, when the tree is in leaf, the sap moves downwards as well as upwards, observing now a circulation.

In Meteorology, if a man be only a good looker, he may one day become an original observer. It is, therefore, a favourite study with those fond of Natural Science. Accordingly, we find upwards of twenty communications, and not a few of them very valuable, on the subject of meteorology in the Society's early Proceedings.

Playfair in 1784 acutely investigates the causes which affect the accuracy of Barometric Observations. In 1790 Dr Rutherford describes a self-registering thermometer, by which the maximum

and minimum of temperature may be ascertained between any given periods of time; and this is still the method usually preferred for ascertaining the daily maxima and minima. In 1795, Mr Alexander Keith, afterwards our benefactor Sir Alexander Keith of Ravelstone, describes both a thermometer and barometer, which, by the application of the same kind of contrivance to each, may be made to register the state of these instruments continually, with the aid of clock-work. Mr Macgowan contributes meteorological observations for six years, ending with 1776, made at Hawkhill, near Lochend, in this neighbourhood; the Duke of Buccleuch, President of the Society, contributes the observations of ten years, ending with 1783, made at Branxholme, his seat in Roxburghshire; and Playfair adds abstracts of observations, made at Windmill Street, in the city, for six years, ending with 1798. In 1796, Dr Balfour of the Bengal Medical Service reproduces an inquiry made a good many years previously, in which, by regular half-hourly observations, he was the first to ascertain that at Calcutta, near the equator, the barometer observes a double diurnal revolution of about a tenth or twentieth of an inch, the highest positions being at ten in the forenoon and evening, and the lowest at six in the morning and afternoon; and in 1799 Playfair points out that this result had been also obtained in 1785 by independent observations made near the equator by Lemanon, a naturalist attached to the ill-starred expedition of La Peyrouse.

Mr Hall of Whitehall describes a remarkable lunar halo, consisting of a small concentric ring, of about ten degrees in diameter, round the moon, and a great ring, seven or eight times that diameter, which passed through the moon, cutting the concentric halo in two. Playfair describes a rare rainbow which he saw over the sea from Dunglass, consisting of a lofty perfect primary arch, almost a complete semicircle, and a secondary bow springing from the south limb of the other, and bending outwards in a southerly direction. The Reverend Dr Graham of Aberfoyle notices an Aurora Borealis which he observed at that place in the day-time on February 10, 1799, at half-past three, and states his belief that this was only the second on record, but conjectures that such observations would not be infrequent, if frequently searched for in the circumstances he describes—viz., “when the sky, being for

the most part cloudless, is suffused with thin pale vapours especially in longitudinal streaks."

I beg here to be allowed to make a short digression. The phenomena of the aurora borealis in this country have often been minutely described on the occurrence of unusually fine displays of it. But no one, so far as I am aware, has studied carefully its prognostications. Thoroughly inquired into, however, these may prove practically valuable, as the following illustration will serve to show. Every one knows that when the aurora first begins to exhibit in the autumn it is regarded as a sign of broken weather following. But at that period of the year it supplies a prognostic of far greater precision and importance. I have repeatedly mentioned to my friends the observation I have invariably made, that the first great aurora after autumn is well advanced, and following a long tract of fine weather, is a sign of a great storm of rain and wind on the forenoon of the second day afterwards. I must have noticed this fact very early, because I applied it on the occasion of the first meeting of the British Association in Edinburgh on 8th September 1834. There had been a long tract of very fine weather, for a fortnight and more, when on Saturday evening, the 6th of the month, there appeared the widest, brightest, and most flashing aurora I have ever seen. Next day the weather continuing remarkably fine, Professor Sedgewick described, at breakfast at Dr Alison's, in glowing language, the magnificent exhibition which the philosophers of Edinburgh had provided for their southern visitors. Presenting then to him the dark side of the picture, I told him that the Association meeting was to be inaugurated with a great storm. He was surprised at this, and appealed to the continuing cloudless sunny sky against me; but I told him the particulars of the prognostication, and that the storm would not begin till the middle of the following day. Next morning the weather was equally splendid. But soon after eleven the eastern sky began to be overcast, an ominous low north-easterly black cloud rose by degrees; at twelve, as the offices of the Association opened, rain began to fall from that direction, and in a short time there commenced the most incessant and heaviest fall of north-east rain I ever witnessed, lasting without intermission till one o'clock on Wednesday the 10th, when the fine weather was again restored to

us and our visitors. I have often made the same prognostication since, and with invariable accuracy; and several friends to whom I have mentioned it have made the same observation—viz., that the first great aurora occurring after a long tract of fine autumnal weather, foretells a storm commencing between twelve and two o'clock in the afternoon of the second day thereafter. I restrict the prognostication to these conditions. It is evident how valuable the knowledge of it may often be to agriculturists. Nevertheless, I never met with farmer or farm-servant who knew it. On one occasion it was the means of saving the corn crop of a friend in Dumfriesshire, whose farm-steward was about to leave his corn half led on the day after a very great aurora, and, deceived by the beauty of the weather, was on the point of taking his labourers to other work not at all pressing. His master, trusting to my positive assurances, ordered him to make haste in leading and thatching everything, and great was the steward's astonishment when a furious three days' storm set in on the forenoon of the second day.

In the pneumatic branch of meteorology three papers were produced to the Society, in the latter part of last century, on topics of great interest to us.

Mr George Wallace, a member of the bar, read, in 1787, "A Dissertation on the Causes of the Disagreeableness and Coldness of the East Wind;" but as the notice in the Society's Proceedings merely informs us that "the author did not incline that any abstract should be given of his dissertation," and I cannot find that it was published elsewhere, we are left in ignorance what were the discoveries or opinions of Mr Wallace on this knotty question, in which we must all take a lively personal interest. It may be that this gentleman has withheld from the world one of the most valuable practical discoveries which remain to be made in meteorology. It may be, however, that he was deterred from having any notice taken of his lucubrations by learning certain views entertained of the same subject by Hutton; who communicated them to the Society in an able paper "On our Vernal and Autumnal Monsoon Winds," in February 1791.

In this paper he points out that winds are shiftings of the air, occasioned by changes in its temperature; that these changes arise from the alternations of day and night, the alternate crossing of the

equator by the sun northward and southward, the different degrees in which the sea and land are heated by the sun's rays, the effect of cloud in interrupting the heating action of the sun, and the influence of rain in cooling the air by the evaporation of its drops in descending towards the earth; and he argues that we might explain all the phenomena of the winds, could we thoroughly appreciate in each instance under investigation the interference and relative energy of these and other subsidiary disturbing forces. He then illustrates these principles of inquiry by reference to our East winds in spring, and our West winds in summer and autumn. For the sake of brevity, I shall confine myself to the former topic, which is investigated with Hutton's characteristic force and simplicity. In spring, says he, easterly winds prevail here, because the cold wintry air of the polar regions is drawn southward over the warmer continent of Europe and westward by the warmth of the German Ocean. For some time after it begins to blow in March and April it occasions in us a cold, uncomfortable feeling, because it is thermometrically cold, and also unduly dry. Its lowness of temperature, compared with the west wind, which occasionally interrupts it for a short period at this season, no one can dispute; and Hutton had frequently remarked a thermometric difference of 10° F. in favour of the interpolated west wind. The cold feeling thus occasioned is aggravated, he says, by the warmth created by the sun's rays in sheltered places; but, in particular, it is increased by the withering dryness of the wind,—for the common notion that an east wind is a damp wind is quite a mistake. During a long period of the spring it is a dry, parching wind, on that account alike disagreeable to the human race and blighting to vegetation. He tells us quaintly that he “never had a hygrometer;” but he improvised one, being nothing else than a somewhat rude wet-bulb thermometer, such as is now in constant use in a more perfect form; and he tells us that in the east winds of early spring he sometimes found a difference of 10° F. between the wet and dry thermometers, while he never could observe a difference of more than 4° F. in the driest days of summer. In the month of May, however, a change takes place in the character of the east wind. Still dominant, it encounters in its passage across the German Ocean a more and more powerful sun, which both

raises the temperature of the air as it approaches this island, and, by warming the sea, raises watery vapour to moisten the lower stratum of air. Hence the May east wind arrives both warmer and less dry. It is now therefore for the most part much less unpleasant to the feeling than in March and April. Towards evening, however, as the sun's rays fail in force, the air, cooling down, parts with moisture to form mist, which creeps up our Firth, spreads over the neighbouring land, prevails all night, and lasts into the next forenoon, till it vanishes under the renewal of the heat of the air from the sun's rays. During the misty time the uncomfortable impression on the human frame is renewed in spite of the air being now moist, partly because the temperature is lowered, partly because mist—being a form of water in the liquid state—is a more powerful conductor of heat from the body than the same water dissolved in the air as gas. Accordingly, as the mist is dispelled with the advance of day, the sense of discomfort vanishes, because the air is warmer, and is loaded merely with perfectly dissolved aëriform moisture. It is remarkable, he adds, that the unpleasant influence of these east winds on man do not then correspond with their influence on vegetation. In March and April the cold, dry east winds wither up and destroy leaves prematurely unfolded. Later in the season an east wind, equally cold, but loaded with moisture, does them no harm, because it loses its parching, withering property. This important difference every careful observer must have noticed. In the case of the animal frame, however, the mere cold of an easterly mist is adequate to cause discomfort, and injury to the health. Thus Hutton maintains that all the disagreeableness and injury occasioned by our east winds, both in spring and in early summer, may be adequately explained by reference simply to their condition in point of temperature and humidity, without our requiring to take account of any other agency, hidden and mysterious in its nature and operation. I have dwelt a little on these simple, yet profound, views of Hutton, because, like other inquiries of his, they have been too much lost sight of in our time, and they may help to clear up and satisfy many minds which have been hitherto much obfuscated and discontent in respect of the present matter.

Hutton appears never to have had an opportunity of studying

hurricanes. If he had, it is not too much to say that his profound penetration into Nature's laws would scarcely have failed to recognise those which govern the mightiest of atmospherical movements. It is not likely, for example, that he would have left in a state of dry detail of bare facts the luminous description by Sir Gilbert Blane, communicated to the Society in 1785, of the terrible hurricane at Barbadoes in 1780 ;—a description containing incidents which nothing but the modern theory of cyclones can explain, and which, duly considered, might have led so acute a mind as that of Hutton to the right solution, even at that early period. This storm lasted the greater part of two days, and raged with unexampled fury for twelve hours, destroying the fort at Bridgeton, levelling an immense number of houses, laying waste the whole crops of the island, and occasioning the sudden death of at least 3000 inhabitants. Notwithstanding, however, its uncommon swiftness, people were surprised at the comparative slowness with which it passed from island to island. They evidently confounded the impetuous whirlwind within the cyclone itself with the slower progress made by the whole cyclone from place to place. Ships at sea, adds Sir Gilbert, found that the storm blew from all points of the compass ; a phenomenon explicable only by the theory of the cyclone. The fate of one vessel is particularly mentioned as unaccountable ; for being blown from her anchorage in Carlisle Bay, and losing all her compasses, she was driven before the blast, as her crew supposed, to a distance of at least a hundred leagues in two days, when, to their astonishment, they found themselves very near the place from which they set out. There was enough in these striking facts to direct an acute and inquiring mind to a true theory of hurricanes, as now viewed by meteorologists.

In the department of Thermometric Meteorology, Mr Hall of Whitehall records a precise fact as to the great cold of the winter of 1795, having observed the thermometer so low as -6° F. on the 22d of January in that year. In order to appreciate duly the great intensity of this cold for a Scottish locality, it is necessary to know that the station where the observation was made is ten miles from the sea at Berwick, on the north bank of the Whittadder, certainly not more than 150 feet above high-water level. About this time the winters in Scotland were very hard.

In 1781 Professor Wilson observed in the Observatory Park at Glasgow a cold of -4° Fahr., and in 1780 actually one of -14° Fahr.

Dr Guthrie, one of many Scottish physicians of the time who settled in Russia, describes in a dissertation read to the Society in 1789 a remarkable phenomenon he had observed in the thawing of thick ice. When ice on the river Neva had been reduced by thaw to two-thirds of its thickness, it became so brittle as not to bear even the weight of a dog, though still eighteen inches thick. The cause he discovered to be that the ice is then composed of solid crystals "like organ-pipes," about eighteen inches long, and with scarcely any cohesion among themselves. This appears a structure somewhat analogous on the large scale to the loss of cohesiveness among the minute particles of ice, which gives occasion to the downward descent of glaciers.

The only original inquiry in Thermometric Meteorology produced to the Royal Society of Edinburgh in its early life, is one of great importance in a scientific respect, as well as practically by reason of its bearings on atmospherical thermometric observations. This is the dissertation well known to learned meteorologists, but lost sight of by too many others, of Patrick Wilson, Professor of Astronomy in Glasgow, "On a remarkable cold which accompanies the separation of hoar-frost in a clear air." This inquiry, read in 1784, and carried on during several previous winters, in continuation of researches of the same kind communicated to the Royal Society of London in 1780 and 1781, is the germ which ultimately produced in the hands of Dr Wells the true theory of the formation of dew and hoar-frost; and the author approaches so near that theory as to create regret that, having stepped on the right path, he had not the luck to follow it to the end. Wilson was the first to observe the difference in clear frosty weather between the temperature of snow, hoar-frost, sand, and many other objects, and the temperature of the atmosphere a few feet above. He frequently observed a difference of 4° , 8° , 12° , and even on one occasion 16° of greater cold on the surface of snow, than in the air four feet, or even only two feet and a half above it. He also noticed that this difference is always attended with an increase of weight from the deposition of hoar-frost; that the difference is always greatest when the atmosphere is clearest and stillest; that wind, even in clear

weather, annihilates the difference; and that such is also the effect of the atmosphere becoming hazy, or the sky overcast with clouds. Wilson laboured to account for all these variable phenomena on the assumption that the deposition of hoar-frost occasioned the cold. But it is easy to see that, in spite of some ingenious suggestions upon that basis, he did not succeed in satisfying entirely his own mind. Had he begun with the converse assumption, that cold was the cause of deposition of hoar-frost, he would probably have anticipated more modern discovery. He was indeed very near doing so. For, speaking of the influence of a passing cloud in putting an end to the formation of hoar-frost and depression of the thermometer, he uses these words, "When the atmosphere becomes suddenly clouded, it is certain that this change must be attended with the extrication of much sensible heat in the higher regions, where these vapours are congregated. A store of heat so produced must soon affect the mass of air which lies below." But how? he might have asked himself. Not surely by the process of conduction, because heated air rises; it does not sink. Not, then, by the process of conduction, but by that of radiation, which, instantly darting heat from the clouds, replaces the loss which in clear weather is sustained by snow and other objects on the surface of the earth through radiation of heat from them into the cold attenuated atmosphere of the far firmament. Unfortunately, however, the theory of radiant heat was too little advanced to suggest to him this explanation,—obvious and easy after the discoveries of Pictet in 1790, and the admirable researches of Leslie fourteen years later.

No one could investigate carefully the theory of the winds without having his attention directed to the Theory of Rain; and, accordingly this is one of the branches of meteorology which Hutton was the first to investigate successfully. This he did in a dissertation read in 1784 and enforced in 1787, in reply to adverse criticisms by the philosopher De Luc. The power which the atmosphere possesses of dissolving or suspending moisture in invisible vapour as transparent as the air increases with the temperature. Hutton's discovery was the proof, by mathematical demonstration, that rain or mist could not be formed when two masses of air of different temperatures are mingled, unless the power to dissolve moisture

increased in a greater ratio than the increase of temperature. Hutton's mathematical deduction has been since proved experimentally to be true. As the solvent power of atmospheric air increases in a greater ratio than that at which the temperature rises, when two masses of transparent air of different temperatures are mixed together, more moisture is present than suffices to saturate the mixed air at the intermediate temperature which is produced; and hence the excess must separate in the form of either mist, if the excess be slight, or rain, if the excess be considerable.

In the topographical branch of Natural History the early Proceedings of the Society present several papers which must have possessed at the time much interest, such as an account of the Caves of Elephanta by Dr Buchanan, of Prince of Wales' Island by Mr Howison, of the Trinidad Petroleum Lakes by Mr Lochead, and of the Natural History of Guiana, and of Madeira, by the same gentleman. But these narratives have been rendered obsolete by more elaborate descriptions published since.

It remains for me, under the head of the Natural Sciences, to take notice of the early labours of the Royal Society in Mineralogy and Geology. In this department the Society, during the first twenty years of its life, shone with a brilliancy unsurpassed by any of the scientific academies of Europe; for during that time were produced Hutton's Theory of the Earth, and the illustrative experiments of Sir James Hall.

The Society's papers on Mineralogy and Geology are eleven in number, but of three of them the Proceedings contain not even an abstract.

Colonel Imray, in a well-told description of the "Mineralogy of Gibraltar" in 1797, corrects some prevailing errors as to the species, composition, and geological position of the famous deposit of bones in various parts of the Rock, and is, I believe, the first to point out that the hill must have been at one time for a long period covered by the sea to the height of at least 900 feet, as he found at that elevation numerous "pot-holes," formed by trituration under water with shingle-stones kept in motion by currents.

Dr Richardson, in 1803, describes three remarkable basalts which

he found on the coast of Antrim, not far from Portrush: one an ochrous basalt, apparently undergoing decomposition; another containing fossil shells, but a dubious basalt; and a third containing bladders of compressed liquid. "This basalt," he says, "contains small cavities in its interior, many of them full of fresh water, which gushes out when the stone is broken by the hammer, as if it had been in a state of compression." Here we have, I apprehend, one of the earliest notices of the presence of liquids in the interior of perfectly solid minerals.

In 1791 Dr Hutton explains the cause of the flexibility of the Brazilian stone, or flexible sandstone. "When a stone," says he, "of any considerable thickness is said to have flexibility, we are led to think that here is something very extraordinary, and we wish to know upon what depends that quality, nowise proper to a stone." Accordingly he set about inquiring, and, after being for some time much puzzled with his problem, he considers that the property is owing to a certain structure, recognisable only with the aid of the microscope, constituted by minute particles of thin mica thickly disseminated through the mass, and always parallel to one another, by which a certain jointed character is given to the stone.

The Rev. Mr Christopher Tait, minister of Kincardine on Forth, has delineated with much care the condition in 1792 of the great Flanders and Kincardine mosses in Stirlingshire and the adjacent eastward coasts of the Firth. Extending on both sides of the Firth, from the line of Kincardine westward as far up the Carse of Stirling as Cardross, the unredeemed desert of peat covered in 1770 a territory 22 miles long, between three-fourths of a mile and seven miles wide, and not less probably than 30,000 acres, assuming an average width of two miles, which I take to be within the mark. He describes the composition of the moss, notices an ancient corduroy road through part of it, shows that at one time its place must have been occupied by a forest of great trees, proves that these had been mostly cut, probably by the early Roman invaders, in order to destroy a retreat and place of assembly for their native enemies, and gives a good succinct account of the famous design of Lord Kames, which had been in successful operation during twenty-two years, for clearing away the peat, uncovering the underlying soil, and converting the moss into agricultural fields. I wish

I could state in contrast, which must be very great indeed, the present condition of that territory after seventy-six years more of enterprise. My limited time has not sufficed to ascertain this point.

I do not know at how early a period we possess a scientific record of the Comrie earthquakes; but there is a precise one by Mr Ralph Taylor in the Society's Transactions, read in 1790, with additions in 1793, describing several visitations, but especially one in 1789, during market-day on 10th November, which made the earthenware vessels clatter in the market-place, terrified horses, and caused the people to think the surrounding mountains were falling on them.

In March 1785 Dr Hutton commenced the reading of his theory of the earth, under the title of "Investigation of the Laws observable in the Composition, Dissolution, and Restoration of Land upon the Globe." The Huttonian theory may be shortly stated as follows:—

Providence, for the wise purpose of preserving and maintaining the excellence of its works, has ordained that all creation, so far as we can study it, shall be subject to alternate decay and renovation arising out of that decay. For this purpose are provided suitable materials and the necessary forces. The earth itself is not excepted. Its present stage of decay is obvious to all eyes on surveying its surface; a prior renovation is almost equally obvious on examining into its crust; and a decay antecedent to that renovation is abundantly evident on careful inquiry in the same quarter.

Under the force of the waves, currents, and alternate flow and ebb of the sea, we may observe that the waters are gradually stealing upon the land, sweeping into their depths the waste so occasioned; but much more under the action of alternate frost and thaw, rains and winds, rivers and floods, earthquakes and other forces, we may behold a never-ceasing wear, slow indeed, but continual and universal, going on over the surface of the dry land; the result being that the waste, along with that of animal and vegetable forms, is constantly carried by the rivers into the ocean. In the depths of the ocean the waste settles down in stillness; and we may safely assume that it settles in the shape of layers, varying in nature and kind, in different ages and at different places, with the prevailing soil, rocks, and vegetable and animal remains from

whose wear upon land the waste has been derived. So much we may discover from what is now going on before our eyes.

If we next assume that at some former epoch a similar waste had been carried on, so long and so far that the earth became less and less suitable for the maintenance of vegetable and animal life on its surface, we then find that a provision exists for renovation of that surface to its pristine condition through the agency of subterranean fire. This power, of whose existence in tremendous energy we have sensible proof in many regions of the globe, had only to approach the subaqueous beds of waste matter in order to fuse them; and then, with a little accumulation of force, to raise them high out of the ocean from its bosom, and even to burst through them, driving far upwards into light and air immense masses of fused materials, long pent up in that state deep in the interior of the earth.

In conformity with this theory, we should find in the present dry land the stratiform portion of the earth's crust presenting in its deepest beds a crystalliform structure corresponding with the laws which govern concretion from a state of perfect fluidity under very slow cooling. On approaching the present actual surface from these deep beds, we ought to find in the superimposed beds rocks presenting signs of agglutination merely, from softening rather than downright fusion. Close to the present surface we should remark perhaps little more than that amount of loose cohesion among the particles composing the rocky beds, which may be fairly ascribed to mere compression sustained while the beds lay under an enormous mass of superincumbent sea. We ought also to remark, that the remains of animals and vegetables are most distinct and least altered by heat from below in the uppermost layers of the stratiform rocks, less and less so as we descend, and at length unrecognisable in the deepest, most perfectly fused beds,—not because vegetable and animal remains were not there deposited along with the earth's waste, but simply because their form was entirely destroyed, and their substance completely incorporated through perfect fusion with the matter in which they lay. If cavities be formed from any cause in the beds of consolidated rocks, we should expect to find in the deep, highly fused beds, but not in those of stony matter agglutinated by mere softening, that these cavities

are lined with crystals in point of composition much simpler than the matrix, or even consisting of a single element only, and yet deriving their ingredient or ingredients from the matrix—such being a law generally of crystallisation from composite mixtures in a state of fusion or of solution. We should moreover expect to find where the melted matter itself of subterranean fire has burst upwards through the stratiform rocks, that it carried broken masses of these rocks along with it, and on cooling showed them included in the invading and now concreted liquid. We should see the disrupting liquid mass diffusing itself in veins in every direction through the shattered beds, and even insinuating itself between the beds, separating them from each other, and now itself forming beds betwixt them. We might also expect to see in the huge masses of subterraneous matter which had been thrown up in a melted state, that, by virtue of their perfect fusion and very slow cooling, they now present us with the most crystalline texture of all rocks, show even a separation into crystalline bodies totally different from one another in composition, and display, in accidental cavities in their interior, lining crystals of the most perfect form, and of the simplest materials which the matrix of rock can yield.

Now, all the phenomena thus described, as it were by anticipation, we do actually witness on examining carefully the several rocky beds and masses which form the present crust of our earth. Therefore there can be no doubt that this crust has been formed from the crumbling of a more ancient dry land, deposited in the sea, and afterwards fused and raised above the sea by the agency of subterranean fire.

Such is in brief terms a summary of the main points and proofs of Hutton's theory, beautifully but tersely set forth in his own dissertation in our Transactions. I might have taken this summary from the admirable extension and illumination of the theory in the classical work of his pupil and friend, Professor Playfair, "*The Illustrations of the Huttonian Theory of the Earth*;" but I have preferred to use for the purpose Hutton's own exposition, because, though he has often been called obscure, I cannot find obscurity anywhere when he is read with the light of details since acquired to science, and probably not altogether unknown to him, notwithstanding that for brevity's sake he has not put them forward.

Two subjects of reflection have been forcibly brought before me in lately renewing, after a long interval of time, my acquaintance with Hutton's own treatise. Firstly, Though he was cultivating a field of inquiry almost entirely new, and developing a vast multitude of new argumentative facts and views, there is scarcely a proposition made from first to last to which a well-instructed modern geologist may not give his assent. Secondly, In his essay of ninety-six quarto pages, he has given his successors in all branches of science a remarkable lesson—a most luminous narrative on a most novel subject, without coining a single new term, or quitting plain English words, unless in the case of a very few German names for rocks previously in universal use among geologists. Much of the repulsiveness of many branches of science to the general student of the present time is no doubt owing to the apparent necessity of a recondite and mysterious nomenclature. Hutton has shown that the most novel and profound inquiries may be propounded with precision, in his day at any rate, without such aid.

The Huttonian theory, though welcomed by many able proselytes, likewise encountered not a few equally able adversaries. These belonged chiefly to the followers of Werner, or Neptunians, who recognised nothing but the force of water in all the apparent revolutions on the earth's surface. The controversy against Huttonianism was for several years carried on at the meetings of this Society with great talent and energy under the leadership of Professor Jameson, a favourite pupil of Werner himself, and the greatest adherent ever gained to the side of that philosopher. Huttonianism may therefore be truly said to have attained its highest triumph when, in the apartment where we are now met, Jameson, not many years before his death, as some of us must well remember, publicly renounced the creed he had taught for half a century, and paid an uncompromising tribute to the truth and profoundness of the Huttonian theory of the earth.

While Hutton's theory was undergoing probation in its early days, some rather troublesome objections were brought against it by its ingenious adversaries. Among these may be here mentioned two, and two only, because they directly gave birth to certain able experimental researches by Sir James Hall, who became our second

President, about twenty years later, on the death of Henry Duke of Buccleuch, in 1811. It was objected that many of the rocks, whose structure was ascribed to fusion by Hutton, become, on cooling from fusion, a slag or a glass, and cannot afterwards recover their crystalline texture. It was also objected that carbonate of lime, which constitutes a large proportion of the stratiform beds in the crust of the earth, cannot be fused, because, long before the heat is raised high enough, it parts with its carbonic acid and becomes lime, which proves refractory under the most intense artificial heat which can be applied. Hutton's answer was that Nature's operations in this matter are carried on upon a vast scale, with unlimited heat, and under enormous pressure—three conditions wholly unlike those in which all experimental imitations must be attempted. Hutton, in his Dissertation seems to have anticipated these objections. But though a skilful and inventive chemist, he did not venture to meet them by experimental evidence. He even threw discouragement over the proposals of his enthusiastic disciples to find a reply by daring to drag Nature into their laboratories. "What!" said he, in a different essay, "judge of the great operations of the mineral kingdom from having kindled a fire and looked into the bottom of a little crucible!"

Sir James Hall, however, resolved not to be thus discouraged. Such was his veneration for his friend and teacher, that he tells us he would not execute his plan during the lifetime of Hutton. But after Hutton's death Hall kindled his fire, and looked into his little crucible; when behold! Nature at work there, exactly as in the vast profound.

In a paper on granite, read to the Society in 1790, he pointed out, that, although the quartz, felspar, and mica, which make up that rock, are fused into glass by artificial heat, there is no reason why, under slow cooling, the crystals of felspar, quartz, and mica should not separate and crystallise apart, in the same way as the crystalline particles of salt and ice separate in the freezing of seawater, or like the crystallisation which renders transparent glass an opaque, rock-like body, when from a state of fusion it is made to consolidate very slowly by gradual cooling.

Sir James tells us afterwards that, at the time this paper was

read, undeterred by the taunts of his friend, he determined to subject these opinions to the test of experiment. The issue was his essay in 1798, entitled "Experiments on Whinstone and Lava." He first by fusion and quick cooling obtained a black vitreous mass from basalt, greenstone, porphyry, and greywacke; and on again fusing these glassy bodies, and cooling them very slowly, he recovered stony masses, "entirely crystalline, with facets appearing in the solid parts," much resembling the original rocks. In the case of basalt from our Castle rock, the resemblance was "so strong in colour and texture, that it would be difficult to distinguish them." Extending these experiments to lava, he tells us, in the first place, that travellers have given rise to erroneous ideas of the characters of lava, by bringing away with them only the superficial scorixæ, and that the deeper parts present very much the appearance and texture of our trap rocks. Specimens of this kind he accordingly found to comport themselves exactly like greenstone and basalt, according to the rate of cooling. The lava of Mount Etna, near Catania, he found to resemble closely the columnar basalt of Arthur Seat, and that near Santa Venere was very like the basalt of the Castle rock; and both of them presented the same varying phenomena as these rocks, when fused and then cooled quickly or slowly.

But Sir James Hall's greatest triumph was his subsequent experimental inquiry, not produced to the Society till 1805, and consequently a little beyond the period included in my present sketch, "On the Effects of Compression in modifying the Action of Heat." By a series of difficult, dangerous, costly, but skilfully contrived experiments, he ascertained that carbonate of lime is a fusible body, if, while exposed to intense heat, it be also subjected to powerful pressure, so as to prevent the conversion of its carbonic acid into gas. He also found that, according to the degree of heat, and consequently of approach to perfect fusion, he could produce, under proportionally high pressures, varying from 52 up to 173 atmospheres, the latter of which corresponds to a mile of sea, every essential character which carbonate of lime variously assumes in the mineral world, from the slightly cohesive chalk to the firm solid structure of opaque secondary limestone, the crystalline structure of translucent marble, and even the transparency and rhom-

boidal form of calcareous spar. Sir James valued these striking results chiefly on account of their irresistible bearing on the Huttonian theory. But, however important they may be in that relationship, they possess high intrinsic merit; and it seems surprising that his method of inquiry has not been extended to other substances.

Hutton made a collection of specimens for illustrating his theory of the earth. This collection was presented to Dr Black by Hutton's sister and representative; Black made it over to the Royal Society; and as the Society had determined not to keep up a museum of its own, Hutton's collection was transferred to the Museum of the University. It must be a subject of keen grief to every lover of geological science, and to every man who feels the respect which is due to the great men of former days, that there is great reason to fear that this collection has been lost sight of, and may not now be capable of being identified. It must have been extensive; for I find in the old manuscripts of the Society's Proceedings, that a committee of Fellows, appointed to conduct its transference to the University Museum, asked no less than a twelvemonth to arrange it. We can scarcely doubt that it was transferred; for it certainly has not been in this Society's possession since I became a Fellow. But it has never been displayed in the University Museum; for indeed it was a perpetual complaint of the late Professor Jameson, that he had no space for exhibiting any collection of rocks at all in the museum. Mr Archer informs me that no such collection has yet come to light in the course of the examination of the vast geological accumulations during the keepership of Jameson, now in the National Museum of Science and Art; and that he doubts, from the state of the portion already examined, whether it will be possible to identify any special part of it. The Society will confidently rely on his zeal and care in this matter; and I submit whether it is not the Society's duty to consider what it can do to save geology from a calamity so deplorable and so discreditable to the scientific history of Edinburgh, as would be the loss of Hutton's own collection which illustrated the Huttonian theory.

The summary now given of the Proceedings of the Royal Society of Edinburgh during the first twenty years of its existence, does not

include a few papers of a kind which cannot be well brought under any of the groups adopted in the preceding arrangement. But some of these merit attention.

Dr Donald Monro describes in 1783 the mode at that time followed in India for obtaining attar of roses without having recourse to distillation. This method consists simply in exposing for six or seven days to the sun picked rose-petals, merely covered with water, in an earthenware jar, and removing with a pellet of cotton on a stick, the volatile oil which gradually forms on the surface. Roebuck communicates in 1798 experiments on the effects of compressed air, made by him in the air-vault of the blowing apparatus of the Devon Iron-works. Only one man was found so venturous as to go with him into the air-vault while the steam-engine was working. The vault was 72 feet long, 14 feet wide, and 13 feet high; and the compressing force was 2.75 pounds per square inch, by which a mercurial column was raised between five and six inches, adding, therefore, nearly a fifth to the mean pressure of the atmosphere. He observed a sense of pressure on the ears, great increase of sound, no perceptible augmentation of heat, but a damp sensation which speedily passed off. No inconvenience was felt during the hour that the experiment lasted. Sir James Hall, in a dissertation on the Origin of Gothic Architecture, deduces ingeniously the Gothic arch from the form assumed by tree poles bent at the top to meet or cross one another, and the florid ornaments of the arch from spontaneous fractures and outward bending of the tree bark on the curves of the arch as the bark dries. Sir Gilbert Blane endeavours to trace the Arabic figures for numerals to India. Lord Ancrum proposes improvements in the arms and accoutrements of cavalry, principally for lessening the weight on the horse, and facilitating the movements of the horseman. Clerk of Eldin shows his well-known nautical propensities, by proposing a scheme for raising sunken ships.

According to custom, I annex a statement of the changes which have taken place during the last twelve months in the membership of the Society.

In November 1867 the Society consisted of 56 Honorary Fellows

and 288 Ordinary Fellows. During last session 20 new Ordinary Fellows have been elected, viz. :—

Rev. David Aitken, D.D.; Robert Daun, M.D.; Rev. D. T. K. Drummond, A.M.; Robt. M. Ferguson, Ph.D.; J. Sampson Gamgee, M.C.S. Eng.; Rev. Jos. T. Goodsir; Colonel Seton Guthrie; Rev. Thomas Guthrie, D.D.; Thomas Key, Dep. Insp.-Gen. Indian Army; J. W. Laidley, Esq., Sea Cliff House; Thomas Smith M'Call, M.D.; J. F. Maclellan, Advocate; John Macmillan, M.A.; Rev. J. F. Montgomery; John Dick Peddie, Esq., Architect; Samuel Raleigh, Esq., C.A.; Adam Gillies Smith, Esq., C.A.; John J. Stevenson, Esq.; Major J. H. M. Stewart; W. Williams.

During the same period two Honorary Fellows and nineteen Ordinary Fellows have died; and one Ordinary Fellow has resigned, viz. :—

Honorary Fellows.—J. B. L. Foucault, and C. F. Schönbein.

Ordinary Fellows.—James Anstruther, W.S. [died in 1866, but omitted last year]; Professor G. A. Walker-Arnott; Principal Sir David Brewster; John Burt, M.D.; Henry Cheyne, W.S.; Right Hon. Sir Geo. Clerk, Bart.; John Davy, M.D.; Rt. Hon. Lord Dunfermline; Robert Hamilton, M.D.; Wm. Bird Herapath, M.D.; Rev. Professor Robert Lee, D.D.; Professor Macdougall; Patrick B. Mure Macredie, Advocate; Thomas Mansfield, Accountant; Dr Manson, Nottingham [died some years ago, but hitherto omitted]; R. Mayne, Esq., Indian C.S.; James Richardson, Esq.; Alex. Thomson, Esq. of Banchory.

Resigned.—Robert Campbell, Esq.

The Society will observe that this obituary includes many names of great distinction in learning and science. In other circumstances it would have been a great pleasure, as also I feel it to have been my duty, to present to this meeting a sketch of the life of these deceased Fellows. But, called on to prepare my address, with brief time to do so, and overwhelmed, too, with unusual University duty, as in charge of the election both of our Chancellor and of our first member of Parliament,—it has been altogether impossible for me, even with the assistance kindly proffered by members of Council, to do anything like justice to such biographies as those of Sir David Brewster, Professor Walker-Arnott, Dr John Davy, Dr Bird Herapath, Dr Robert Lee, Dr Manson, Sir George Clerk, Mr Thomson of Banchory, and others of the Ordinary

Fellows whom we have lost ; or those of our late eminent Honorary Fellows, Schönbein and Foucault. A full biography, however, of our late President, Sir David Brewster, will appear ere long from some well-qualified pen, and one of Professor Walker-Arnott by Dr Cleghorn.

The following Gentlemen were elected Foreign Honorary Fellows of the Society :—

GUSTAV ROBERT KIRCHHOFF, Professor of Physics in the University of Heidelberg.

RUDOLPH VIRCHOW, Professor of Pathological Anatomy in the University of Berlin.

Monday, 21st December 1868.

The following Communications were read :—

1. On the Colour of Aërial Blue. By Sir George Harvey.

This paper is intended to prove that the colour of blue in the sky and in the landscape is simply the result, in the former, of the darkness of space, as seen through the white light contained in the atmosphere ; and, in the latter, of the same cause as shown in the dark and distant portions of the landscape being viewed through the interposed medium of air filled with white light. The colour of aërial blue being due in both cases to the same cause, namely—a dark body neutralised as to its darkness by being seen through a white and transparent medium.

2 On the Rotation of a Rigid Body about a Fixed Point.

By Professor Tait.

(Abstract).

This paper contains an attempt to exhibit the mutual relations of some of the more important of the various processes which have been employed in solving one of the most celebrated problems of Dynamics. The Quaternion analysis has been throughout used, as far more briefly comprehensive and more suggestively expressive than the ordinary Cartesian analysis.

A brief sketch of the *kinematical* relations of the problem is first

given, partly after Hamilton; and in it the main object sought is usually the quaternion, q , on which depends the operator

$$q(\quad) q^{-1}$$

which turns the body from any initial position whatever to its position at time t . The investigation of the axis and amount of the single rotation by which the body may be thus changed in position was first suggested by Euler, but it was greatly simplified and extended by Rodrigues and Cayley. The fundamental kinematical formula of the present paper, which connects the quaternion, q , with the instantaneous axis of rotation, ϵ , is

$$\epsilon = 2V\dot{q}q^{-1},$$

and had been obtained by Cayley, though not in this very simple form, as a quaternion translation from some of his Cartesian results.

From this equation the formulæ, connecting the angular velocities about the principal axes with the various sets of three angular co-ordinates which have been employed to determine the position of the body at time t , are deduced, mainly to show how complex are these systems as compared with those suggested at once by quaternions.

Hamilton has pointed out that, if ϖ be the vector of an element m of the mass, the whole *kinetic* properties of the motion are contained in the equation (which is really that of Lagrange)

$$\Sigma . m V \varpi (\ddot{\varpi} - \psi) = 0,$$

where ψ is the vectorex pressing the applied force on unit of mass at m . He has also given the *kinematical* relation

$$\dot{\varpi} = V \epsilon \varpi.$$

By means of this he obtains

$$\Sigma . m \varpi V \epsilon \varpi = \gamma,$$

where γ is a constant vector if no forces act, otherwise it is the time-integral of the vector-couple

In the paper it is proved that if we write

$$\eta = q^{-1}\epsilon q$$

$$\zeta = q^{-1}\gamma q$$

(where η and ζ are certain vectors in the body in its initial position) the whole kinetic properties of the motion are expressed by the equation

$$\phi\eta = \zeta,$$

where ϕ is a *linear and vector function*, which here introduces (as the roots of its determining cubic) the three moments of inertia.

As the tensor of q may have any value whatever, let

$$Tq = \text{constant}.$$

Then our equations become

$$q\eta = 2\dot{q},$$

$$\gamma q = q\zeta,$$

$$\phi\eta = \zeta.$$

On the integration of these very simple forms the solution of the problem depends. They give

$$q\phi^{-1}(q^{-1}\gamma q) = 2\dot{q}$$

as the quaternion equation for q ; where, however, if forces act, γ is to be considered as a function of q ; and they supply the counterpart of Euler's equations in the form

$$\phi\dot{\eta} = -V\eta\phi\eta,$$

when γ is constant.

If we seek the actual equations of Euler, referred to the moving principal axes, we obtain

$$\phi\dot{\epsilon} = -V\epsilon\phi\epsilon,$$

where ϕ differs from ϕ simply in the fact that its rectangular unit-system is fixed in, and moves with, the body.

If we write

$$q = w + ix + jy + kz$$

the equation above (for q) gives us the following set of ordinary

differential equations containing the complete solution of the problem when no external forces act :

$$\frac{dt}{2} = \frac{dw}{W} = \frac{dx}{X} = \frac{dy}{Y} = \frac{dz}{Z},$$

where

$$\left. \begin{aligned} W &= -x\mathcal{A} - y\mathcal{B} - z\mathcal{C} \\ X &= w\mathcal{A} + y\mathcal{C} - z\mathcal{B} \\ Y &= w\mathcal{B} + z\mathcal{A} - x\mathcal{C} \\ Z &= w\mathcal{C} + x\mathcal{B} - y\mathcal{A} \end{aligned} \right\}$$

and

$$\begin{aligned} \mathcal{A} &= \frac{1}{A} \left(a(w^2 - x^2 - y^2 - z^2) + 2x(ax + by + cz) + 2w(bz - cy) \right) \\ \mathcal{B} &= \frac{1}{B} \left(b(w^2 - x^2 - y^2 - z^2) + 2y(ax + by + cz) + 2w(cx - az) \right) \\ \mathcal{C} &= \frac{1}{C} \left(c(w^2 - x^2 - y^2 - z^2) + 2z(ax + by + cz) + 2w(ay - bx) \right). \end{aligned}$$

Here A, B, C are the principal moments of inertia, and

$$\gamma = ia + jb + kc$$

is the constant vector of moment of momentum.

Thus we see that W, X, Y, Z are *homogeneous* functions of w, x, y, z , of the third degree. Equations of this nature, but not so symmetrical, have been given by Cayley, and completely integrated (in the sense of being reduced to quadratures) by assuming the previous integration of Euler's equations.

Other modes of integration are employed; and the problem is also solved by seeking the *homogeneous strain* which will bring the body from any initial position to its position at time t .

This part of the paper concludes with the complete determination of q for the case of no forces and two equal moments of inertia.

The remainder of the paper deals with some simple cases of applied forces, when two moments of inertia are equal. If α denote a unit vector in the direction of the unequal axis of inertia, and if the motion be that of a heavy solid of revolution (such as a top) about a point in its axis, it is shown that

$$BV\alpha\ddot{\alpha} - A\Omega\dot{\alpha} = V\alpha\gamma$$

where γ is a constant vertical vector, and

$$\Omega = S\alpha\epsilon = \text{constant}.$$

This is the equation of motion of a simple pendulum disturbed by

a force constantly perpendicular to the cone described by the string, and proportional to the rate at which the area of the surface of the cone is swept out by the string. The locus of the extremity of ϵ is shown to be a sphere fixed in space.

The problem of Precession and Nutation is next considered, and shown to depend on the integration of the very simple equation

$$BVa\ddot{a} - A\Omega\dot{a} = \frac{3M}{T^2\rho} (A - B) S_{ap} V_{ap}.$$

where M is the mass, and ρ the vector, of the disturbing body.

The complete developments of the solutions of these equations are reserved for another occasion.

3. An Investigation into some previously undescribed Tetanic Symptoms produced by Atropia in Cold-Blooded Animals. By Dr Thomas R. Fraser.

Authorities in toxicology appear to agree in including convulsions among the effects of belladonna and of its active principle—atropia—on man. Convulsive and tetanic symptoms would appear to be also nearly constantly produced when fatal doses of this poison are administered to dogs, rabbits, and other animals, and to various birds. The recent remarkable progress of our knowledge of the exact and intimate physiological action of various medicinal substances is greatly due to investigations that have been made on animals of a lower type of organisation; and, accordingly, numerous observers have instituted experiments with atropia on such animals, and, especially, on frogs. Hitherto, however, tetanus has not been described as one of the effects of atropia-poisoning in cold-blooded animals.*

In some experiments undertaken to determine the minimum fatal dose of atropia for frogs, I was surprised to find that increased reflex excitability, convulsions, and tetanus occurred, occasionally, at a certain stage in the poisoning. Since first observing these unexpected symptoms, I have made a number of experiments to

* Since this was written, I have communicated with Dr John Harley of London (the author of several important papers on the physiological action and therapeutical employment of belladonna), and have had the pleasure of learning that he has also observed tetanus, and other symptoms of abnormal reflex activity, in frogs during protracted atropia-poisoning.

determine, accurately, the character of these convulsive effects, to ascertain the dose necessary for their production, and to differentiate, as far as possible, the structures on whose affection they depend.

Soon after a small fatal dose, or one rather less than fatal, of a salt of atropia is administered to a frog, a slight degree of weakness occurs in the anterior extremities, the respiratory movements of the chest cease, and the motor power becomes gradually more and more impaired, until, at length, all voluntary and respiratory movements cease, and the animal lies on the abdomen and chest, in a perfectly flaccid state. If the condition of the heart be now examined, it will be observed that the cardiac impulse is scarcely perceptible, and that the contractions are reduced to a very few in the minute. At this time, the application of various stimuli shows that the functions of the afferent and efferent nerves and of the spinal cord are retained, though in a greatly impaired condition. Several hours afterwards, it may be on the following day, the action of the poison is still further advanced; for the functions of the afferent and efferent nerves and of the spinal cord are completely paralysed, while only an occasional and scarcely perceptible cardiac impulse can be discovered, the only signs of vitality being this imperfect cardiac action and the continuing irritability of the striped muscles. This state may last for many hours or for several days—in one experiment it continued for as many as five days. Previous observers have apparently mistaken it for one of death, and have, therefore, failed to observe the symptoms that subsequently appear, and to which I wish more particularly to draw attention. The first of these symptoms is, usually, a change that occurs in the flaccid condition of the animal; the anterior extremities becoming flexed, and gradually more and more arched, until, at length, they are rigidly contracted, while tonic spasm occurs in the muscles of the chest also. At this time, a touch of any portion of the skin increases the tonic spasm of the anterior extremities and of the chest muscles, and causes some slight spasmodic movements in the posterior extremities. In varying periods after this, the respiratory movements reappear, and the cardiac impulse improves greatly in strength and in frequency, while the posterior extremities assume an extended position, with the webs stretched. If the skin be now touched, a violent attack of opisthotonic tetanus occurs, which may last for

from two to ten seconds, and which is succeeded by a series of clonic spasms. During the tetanus, the posterior extremities are often more or less abducted, and immediately after it they become flaccid; but the anterior extremities almost always remain rigidly flexed. As a somewhat later period, tetanus of a still more violent character may be excited; the attacks are now emprosthotonic, and during them, the posterior extremities are rigidly extended, while at their conclusion, not only do the anterior extremities remain arched, but the head is bent downwards by tonic spasm of the muscles of the abdomen, chest, and neck.

A succession of such attacks may be produced by repeated touches of the skin, but, after a number have been excited in quick succession, the subsequent convulsions become shorter and rather less powerful, though they reacquire all their former violence after a period of rest.

When the animal is not suffering from an attack of tetanus, it may execute various movements, but these are performed with difficulty, even when they do not themselves excite spasms and convulsions, and it is apparent that the power of voluntary movement is still considerably impaired.

The period during which this tetanic condition remains was found to vary greatly in different experiments. It has been observed to continue for only a few hours, or for several days, and, in one experiment, for as long as fourteen days.

This description indicates the usual characters and sequence of the phenomena with such a dose of atropia as produces tetanus. Experiments have, however, been made in which the functions of the cerebro-spinal nervous system were not observed to be completely paralysed, in the stage of the poisoning antecedent to the appearance of tetanus. Only impairment of these functions was observed, but, as the state of flaccidity often lasts for several days, it is obviously impossible to make observations so frequently during this period as to authorize the assertion that total destruction did not occur.

It is almost superfluous to allude to the resemblance between the tetanic symptoms of atropia and those of strychnia. There are, however, certain peculiarities connected with the tetanus which atropia causes—altogether apart from the remarkable fact that this tetanus succeeds paralysis—which distinguishes it from that of

strychnia. After poisoning with atropia, and during the stage of exaggerated reflex excitability, the attacks of tetanus cannot be excited by the very slight stimuli which are sufficient to do so in strychnia-poisoning. Various irregularities, also, are frequently met with in the tetanus of atropia. Some of these have been already described, and of the others it is sufficient to mention the occurrence of tonic spasm of one group of muscles in one limb, and of another group in another; of tetanus in the posterior extremities, with only slight increase of reflex excitability in the anterior; and of contractions of unequal force in the muscles at the sides of the chest and neck, causing lateral curvature during a tetanic convulsion.

The numerous experiments that have been made have so far solved the problem of the dose required to produce these remarkable phenomena, that they may now be almost unfailingly produced. Tetanus, or, at least, a state of greatly exaggerated reflex excitability, nearly invariably occurs when a dose of the sulphate or acetate of atropia, equivalent to the one-thousandth of the weight of the frog, is administered by injection, either under the skin or into the abdominal cavity. Doses varying from the one eight-hundredth to the one twelve-hundredth of the frog's weight, may also produce these effects. The larger doses always produce the most violent tetanic symptoms, if they are not fatal during the stage of paralysis; and they may be given with confidence to very small animals, and to such as have been kept in a laboratory for several months. The smaller doses are best adapted for large frogs, and for such as have been recently obtained from their natural habitats. If a dose be employed smaller than those above indicated, impairment of the functions of the cerebro spinal nervous system and of the heart may be caused, but general tetanus will not follow, although spasms restricted to certain regions may occasionally appear. The tetanic state resulting from the administration of the largest doses usually terminates in death, that from the smallest in recovery.

There are some special difficulties to be overcome in determining what structures are concerned in the production of this tetanic action of atropia; for in following the only available plan, that, namely, of preventing the poison reaching certain regions while it has access to others, it is essential to remember that important

fallacies might arise because of the long interval that often elapses between the administration of the poison and the appearance of tetanus. Experiments were made in some of which the blood-vessels of one posterior extremity, and in others of both, were tied before atropia was administered, and, by frequently modifying the dose, tetanus was on several occasions produced sufficiently soon to give results that were not materially influenced by the previous ligation of vessels. It was observed, in these experiments, that spasms and tetanus occurred in the limbs to which the access of the poison had been prevented, during the stage in which the nerves of the poisoned regions were regaining their functions. This is sufficient to demonstrate that the tetanus does not depend on an action on motor or sensory nerves, nor on muscles; and it is, therefore, apparent that it must depend on an action on the central nerve-organs. The predominance of cerebral symptoms during atropia-poisoning in animals of a higher development, suggested the possibility of the tetanic symptoms being caused in frogs by an influence originating in the cerebral lobes, or, more probably, in the ganglia at the summit of the medulla. Accordingly, on several occasions, the cord of a frog in the stage of tetanus was divided immediately below the brachial enlargement. After this operation, however, the tetanic condition continued in both the anterior and posterior segments. Violent tetanus could be readily excited in either segment; and this condition frequently lasted for many days after the division of the cord.

There can, therefore, be no doubt that these tetanic symptoms are caused by an action of atropia on the spinal cord.

4. On *Rhabdopleura*, a New Genus of Polyzoa. By Professor Allman.

Professor Allman described a new genus of Polyzoa, obtained by the Rev. A. M. Norman and Mr J. Gwyn Jeffreys, from deep-sea dredgings in Shetland.

Its coenœcium consists of a branched tube, partly adherent and partly free, the free portion forming tubes of egress, through which the polypides move in the acts of exsertion and retraction. In the walls of the adherent portion a rigid chitinous rod is de-

veloped along their attached side, and to this rod the polypides are connected from distance to distance each by a flexible cord or funiculus.

The polypides are hippocrepian, and each carries a shield-like process on the hæmal side of its lophophore external to the tentacular series.

The development of the bud was traced, and it was shown that in an early stage the polypide is included between two fleshy plates, which are placed, one on the right and the other on the left side, and are united to one another along a portion of their circumference, while they are disunited along the rest. For some time the two plates keep pace with the general development of the bud, but ultimately they cease to increase in size, and then remain as the shield-like process carried by the lophophore of the adult polyzoon.

The author regarded these plates as representing the right and left lobes of the mantle in a Lamellibranchite mollusc, from which it followed that the relations of the Polyzoa are more intimate with the Lamellibranchiata than with the Brachiopoda, with which of late years they had been associated, but whose mantle lobes lie dorsally and ventrally, instead of lying right and left, as in the Lamellibranchiata. The lophophore of the Polyzoa was considered by the author as having its representative in the labial palps of the Lamellibranchiata.

The following were given as the generic and specific diagnosis of the new Polyzoon :—

Genus RHABDOPLEURA, Allman.

Cœnæicum consisting of a branched adherent membranous tube, in whose walls along their adherent side a rigid chitinous rod extends, and whose branches terminate each in a free open tube through which the polypide emerges.

Lophophore hippocrepial, with a shield-like process on the hæmal side of the tentacles. Polypides connected to the chitinous rod by a flexible cord or funiculus.

Rhabdopleura Normani, Allman.

Cœnœcium sub-alternately branched, delicate, transparent, and colourless; free portion of the cœnœcial tubes of the same diameter as the adherent portion, and very distinctly and regularly annulated.

Habitat.—Creeping over the surface of dead shells, from a depth of 93 fathoms.

Locality.—Shetland seas. J. Gwyn Jeffreys, Esq., and Rev. A. M. Norman.

Though we cannot expect, in spirit specimens, to demonstrate by direct observation the presence of an epistome, we may yet take for granted that Rhabdopleura, like all other polyzoa with hippocrepian lophophores, is provided with this organ, and belongs to the Order Phylactolæmata. Its structure, however, is so peculiar as to justify us in assuming it as the type of a special section of the Order, which may be thus divided—

POLYZOA PHYLACTOLÆMATA { *Scutata*, Rhabdopleura.
 Inermia, Cristatella, Plumatella, &c.

The following Gentlemen were balloted for and elected Fellows of the Society:—

OLIVER G. MILLER, Esq., Panmure House, Forfarshire.

JOHN L. DOUGLAS STEWART, Esq. of Nateby Hall.

ALEXANDER BUCHAN, Esq., M.A.

PROFESSOR FLEEMING JENKIN.

WILLIAM DICKSON, Esq.

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